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instruments. Tables are appended to the paper, showing the monthly, quarterly and yearly mean temperatures, with those of groups of years, and other tables exhibiting the departure of every individual result from the mean of all.

The author concludes by stating, that hitherto the mean temperature at Somerset House has been estimated a great deal too high. He does not here enter into the investigation as to whether the temperature as now determined is too high for the geographical position and elevation of Somerset House, but proposes to do so, in a paper he is preparing with the view of connecting the Somerset House with the Greenwich series, and of bringing up all the results to the present time. He hopes also, at some future time, to present results from the barometrical observations arranged in a similar manner.

May 10, 1849.

The EARL OF ROSSE, President, in the Chair.

The following communication was read :—" Remarks on M. De la Rive's Theory for the Physical Explanation of the Causes which produce the Diurnal Variation of the Magnetic Declination," in a letter to S. Hunter Christie, Esq., Sec. R.S., from Lieut.-Col. Sabine, For. Sec.R.S. Communicated by S. Hunter Christie, Esq.

MY DEAR SIR,

Woolwich, April 16, 1849.

The *Annales de Chimie et de Physique* for March last contains a letter from M. De la Rive to M. Arago, in which a theory is proposed, professing to explain on physical principles the general phenomena of the diurnal variation of the magnetic declination, and, in particular, the phenomena observed at St. Helena and at the Cape of Good Hope, described in a paper communicated by me to the Royal Society in 1847, and which has been honoured with a place in the Philosophical Transactions.

Although I doubt not that the inadequacy of the theory proposed by M. De la Rive for the solution of this interesting problem will be at once recognised by those who have carefully studied the facts which have become known to us by means of the exact methods of investigation adopted in the magnetic observatories of recent establishment, yet there is danger that the names of De la Rive and Arago, held in high and deserved estimation as authorities on such subjects, attached to a theory, which moreover claims reception on the ground of its accordance with "well-ascertained facts" and "with principles of physics positively established," may operate prejudicially in checking the inquiries which may be in progress in other quarters into the causes which really occasion the phenomena in question; I have thought it desirable therefore to point out, in a very brief communication, some of the important particulars in which M. De la Rive's theory fails to represent correctly the facts which it

professes to explain, and others which appear to me to be altogether at variance with, and opposed to it.

1. M. De la Rive's theory, in a few words, is as follows :—

In consequence of the inequalities of temperature in the higher and lower strata of the atmosphere, electric currents are generated, which in the higher regions proceed from the equator to the poles, and return at the surface of the earth from the poles to the equator; the return current causing in the northern hemisphere the north end of the magnet to deviate in the one direction, and in the southern hemisphere in the opposite direction; the deviation being at any given place greatest at the hour (about 1^h.30 P.M.) when the difference of temperature in the upper and lower strata of the atmosphere is greatest, and of course increasing until that hour, and subsequently diminishing.

That the north end of the magnet does thus deviate in the forenoon towards the west in the northern hemisphere, and towards the east in the southern hemisphere, and return in both cases in the opposite directions in the afternoon, were facts known before the establishment of the magnetic observatories; but M. De la Rive's explanation of them appears to have been suggested, and its appropriateness, as he considers, is shown, by its affirmed accordance with the remarkable peculiarity in the phenomena made known to us by the observations at the Magnetic Observatory at St. Helena, and communicated to the Royal Society in the paper referred to. This peculiarity is briefly as follows: the deviation which constitutes the principal part of the diurnal variation at St. Helena is *not uniform in its direction throughout the year*; in one part of the year it is to the west, and in the other part of the year to the east; and consequently during certain months of the year the movement of the magnet is in the *contrary direction* to that which prevails at the same hours during the other months of the year.

Now St. Helena is situated within the tropics, and M. De la Rive infers from his theory that in all places so situated, the diurnal variation should be in one direction when the sun's declination is north of the latitude of the place, and in the contrary direction when the sun's declination is south of the latitude of the place: and hence he too hastily concludes that his theory accords with the characteristics of the diurnal variation at St. Helena. When however the facts are more closely examined, it is seen that they do by no means accord with M. De la Rive's supposition.

That it may be quite clear that I do not misapprehend either M. De la Rive's theory, or his supposition in regard to the facts at St. Helena, I subjoin his own expressions, which convey his meaning, as that gentleman's writings generally do, with most commendable precision.

The first extract defines the limit which, according to his theory, should separate the electric currents proceeding respectively from each of the poles to the equator; and should consequently separate the parts of the globe in which the diurnal variation is in the one direction, from the parts in which it is in the opposite direction;

whilst the second extract describes what he believes to be the facts of the phenomena at St. Helena.

Extract 1.

“La limite qui sépare les régions occupées par chacun de ces deux grands courants n'est pas l'équateur proprement dit, car elle doit être variable : elle est, d'après la théorie que je développe, celui des parallèles compris entre les tropiques, qui a le soleil à son zénith ; elle change par conséquent chaque jour.”

Extract 2.

“À St. Hélène, la variation diurne a lieu à l'ouest tant que le soleil est au sud de l'île, à l'est dès que le soleil est au nord. En effet, dans le premier cas, ainsi que j'ai remarqué plus haut, St. Hélène doit faire partie de la région dans laquelle les courants électriques vont sur la surface de la terre du pôle boréal aux régions équatoriales ; et, dans le second cas, de la région dans laquelle ces courants vont du pôle austral vers l'équateur.”

Whoever will be at the pains to refer to the paper printed in the Philosophical Transactions, describing the phenomena at St. Helena, or to the volume containing the details of the observations on the diurnal variation in each month during the five years in which hourly observations were maintained day and night at that observatory, will perceive,—on evidence which admits of no uncertainty,—that the two portions of the year in which the diurnal variation is in contrary directions at that island, are not determined, as M. De la Rive supposes, by the declination of the sun relatively to the *latitude of the place*, but by the declination of the sun relatively to the *equinoctial line*. The sun is vertical at St. Helena, passing to the south in the first week of November ; and again when passing to the north in the first week of February ; consequently the two portions into which the year is thus divided, are respectively the one of *three*, and the other of *nine* months' duration ; but the actual portions in which the contrary diurnal movements of the magnets take place at St. Helena are of *equal* duration, and consist of *six* months and *six* months ; the dividing periods coinciding unequivocally, not with the sun's verticality at St. Helena, but with the equinoxes.

2. But if M. De la Rive's explanation be thus inconsistent in respect to the dates of the transition periods of the phenomena at St. Helena, it must be regarded as altogether at variance with, and opposed to, the phenomena described in the same paper at the Cape of Good Hope, where also they have been observed at the Magnetic Observatory at that station with an exactness which leaves no uncertainty whatsoever as to the facts themselves. The Cape is *not* situated within the tropics ; its latitude is $33^{\circ} 56'$ south ; the sun is consequently throughout the year well to the north of its zenith ; and therefore, according to M. De la Rive's theory, the deviations should be in one and the same direction throughout the year. But the fact is not so ; for the same contrariety in the direction of the diurnal variation at different portions of the year takes place at the Cape as at St. He-

lena; the two portions of the year in which the opposite phenomena prevail, are also identical at the two stations; and at both the change in the direction of the deviation takes place when the sun crosses the equinoctial line; the deviation being to the west at both stations when the sun is in the northern signs, and to the east when he is in the southern signs.

3. But in considering a theory which comes before us, claiming the high distinction of affording a physical explanation of facts which are known to us by well-assured observation, we ought not to confine our view to those points only for which it professes to supply the explanation: these are certainly tests as far as they go;—and in the present instance the conclusion from them is not favourable to the theory proposed;—but we should also notice the deficiencies of the theory; or those points wherein it neither furnishes, nor attempts to furnish, explanations of circumstances which are certainly amongst the most remarkable facts of the case. They may be possibly amongst the most difficult to explain; but no physical theory can be regarded as meeting the facts which does not at least attempt an explanation of them. I may name as the most prominent in interest amongst these the striking fact, that the Cape of Good Hope should be one of the stations at which this remarkable peculiarity, of a contrariety of movement at different periods of the year, takes place.

It is known that it does not occur at places situated in corresponding latitudes north of the geographical equator; at Algiers, for example,—which is moreover nearly in the same geographical meridian as the Cape, and where the magnetic inclination is nearly the same towards the north as is the case at the Cape towards the south. It may be quite correct perhaps to view the corresponding phenomena at St. Helena and the Cape as those belonging to *magnetically-equatorial* stations; but they are certainly not those peculiar to or characteristic of *geographically-equatorial* stations, which would be the condition in M. De la Rive's theory. There are thus two parts in the problem which await a physical explanation; on the one hand, the cause is required of the contrariety of movement, as well as of the times at which the different movements occur, the latter having obviously a dependence on the sun's position whether in the northern or the southern signs; and on the other hand, the cause must be shown why certain stations are thus affected and others not: a distinction which obviously does not depend on situation in regard to the geographical equator or to the tropical divisions of the globe.

I have myself been led to infer that the peculiarity in question results from and is indicative of proximity to the line of *least magnetic force*, regarded as physically the separating line on the surface of the globe between the northern and southern magnetic hemispheres; under this explanation the peculiarity would be strictly a magnetically-equatorial one.

It results from the present position of the four points of maximum intensity at the surface of the earth, that the intermediate line of least intensity departs considerably in the Southern Atlantic from the middle or geographically-equatorial portion of the globe, and passes

between the Cape and St. Helena, and consequently not far from either of those stations.

As far as I have yet been able to examine, I have found that the same remarkable peculiarity does exist at all other stations which are near this line, and at none which are remote from it. But however this may be, the accordance of the phenomena at the Cape of Good Hope and St. Helena, and their dissimilarity from those at other stations is a well-ascertained fact, of far too much bearing and importance to be passed without notice; and we may safely anticipate that its cause must occupy a prominent place in the theory which shall be ultimately received, as affording an adequate solution of the problem of the diurnal variation.

Believe me, my dear Sir, sincerely yours,

EDWARD SABINE.

S. H. Christie, Esq., Secretary to the Royal Society.

May 24, 1849.

The EARL OF ROSSE, President, in the Chair.

The following papers were read:—

1. An appendix to a paper "On the Variations of the Acidity of the Urine in the State of Health"—"On the Influence of Medicines on the Acidity of the Urine." By Henry Bence Jones, M.D., M.A., F.R.S. &c.

The variations of the acidity of the urine in the state of health having been shown in the original paper, and the effect of dilute sulphuric acid also traced; in this appendix, the influence of caustic potash, of tartaric acid, and of tartrate of soda, on the acidity of the urine is determined.

One ounce of liquor potassæ, specific gravity 1072, was taken in distilled water, in three days. It hindered the acidity of the urine from rising, long after digestion, to the height to which (from comparative experiments) it otherwise would have done; but it, by no means, made the urine constantly alkaline; nor did it hinder the variations produced by the state of the stomach from being very evident.

354 grains of dry and pure tartaric acid dissolved in water were taken in three days. The conclusion from the observations is that this quantity increased the acidity of the urine, but during that time it did not render the effect of the stomach on the reaction of the urine less apparent than when no acid was taken; and therefore, that this quantity of tartaric acid, during this time, does not produce so much effect on the reaction of the urine as the stomach does.

Tartrate of potash in large doses produces the most marked effect on the alkalescence of the urine. 120 grains of pure dry tartrate of potash dissolved in four ounces of distilled water made the urine alkaline in thirty-five minutes. In two hours the alkalescence had